AN
ATTEMPT
TO PROVE THAT
DIGESTION,
IN MAN,
DEPENDS ON THE
UNITED CAUSES
OF
SOLUTION AND FERMENTATION.

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By JOSEPH GLOVER,
Of Charleston, South-Carolina,
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* * *

> ———— Tentanda via eſt, quà me quoq; poſſim
> Tollere humo. ————
>
> Vir.

* * *

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AN

INAUGURAL EXPERIMENTAL INQUIRY,

FOR THE DEGREE OF

DOCTOR OF MEDICINE;

SUBMITTED TO THE

EXAMINATION

OF THE

REV. JOHN EWING, S. T. P. PROVOST;

THE

TRUSTEES & MEDICAL FACULTY,

OF THE

UNIVERSITY OF PENNSYLVANIA,

On the thirty-firſt of May 1800.

TO

DOCTOR MATTHEW IRVINE,

PHYSICIAN OF CHARLESTON, SOUTH-CAROLINA,

WHO,

IN THE TWO-FOLD CHARACTER

OF A

FRIEND AND PRECEPTOR,

HAS THUS FAR GUIDED ME,

BY HIS

PATRONAGE AND CARE,

THROUGH AN

IMPORTANT BRANCH OF SCIENCE;

THIS

PUBLICATION

IS RESPECTFULLY INSCRIBED,

AS A SMALL TESTIMONY

OF

ESTEEM AND GRATITUDE,

BY HIS MUCH OBLIGED

FRIEND AND PUPIL,

J. GLOVER.

TO

BENJAMIN SMITH BARTON, M. D.

PROFESSOR OF MATERIA MEDICA, NATURAL HISTORY AND
BOTANY, IN THE UNIVERSITY OF PENNSYLVANIA,

THIS

DISSERTATION

IS LIKEWISE INSCRIBED,

AS AN ACKNOWLEDGMENT

OF THE

ATTENTION AND ADVICE

WITH WHICH HE HAS HONOURED

HIS MUCH OBLIGED FRIEND,

J. GLOVER.

PREFACE.

THE difficulty of experimenting, appears to be the reaſon, why phyſiology has not kept pace with other branches of medical ſcience. In moſt of theſe we may travel on a ſmooth and delightful road; but the moſt trivial circumſtances influence the reſult of experiments; thus aſſertion is oppoſed to aſſertion, and on the reputation of the author reſts the poſition.

In the following pages it is contemplated to inveſtigate the proceſs of digeſtion; a ſubject by no means perfectly underſtood. In doing this, I have endeavoured to arrange ſuch ideas on the ſubject, as I have collected from the opinions of others, or from ſuch facts and experiments as I have myſelf aſcertained. Theſe have taught me the difficulty of explaining the phenomena of digeſtion on moſt of the theories which have been advanced; nor do I expect that the one, which I have adopted, is void of imperfections. Too fond of reducing every operation of the animal œconomy to a ſingle principle, many phyſiologiſts have explained the proceſs of digeſtion on ſome favourite theory, and thus, by ſetting a limit to the hand of nature, have left unaccounted for ſome of the moſt important phenomena.

The reſult of my inveſtigations, ſuch as it is, circumſtances have induced me to caſt as my portion into the ſcale of ſcience. Should it ſuggeſt an idea worthy the attention of the philoſopher, I ſhall feel highly gratified; but, on the contrary, ſhould my experiments prove inconcluſive, and error ſtamp the reputation of my eſſay, ſtill I ſhall be pleaſed with the reflection, that the moſt feeble attempt to elucidate this important branch of phyſiology, can be by no means injurious to ſcience.

The haſte, ſo unavoidably annexed to the ſhort time allotted for this publication, will no doubt induce the reader to overlook the many inaccuracies of language, which I am fully aware are contained in the following pages. They may perhaps bring to his recollection, that ſentence of Dr. Beddoes ſo expreſſive of the liberality of the philoſopher, when he ſays, "we ſhould ſet a due value on our "preſent knowledge, though it be imperfect; and "reſtrain thoſe rude hands, that are ever ready to "pluck up the tender plants of ſcience, becauſe "they do not bear ripe fruit at a ſeaſon when they "can be only putting forth their bloſſoms." Impreſſed with the generoſity of theſe ideas, I ſubmit my obſervations and experiments to the candid inveſtigation of the philoſopher, whoſe patronage alone ſtamps the merit of every youthful performance, gives activity to the mind, and ſanctions future inveſtigations.

AN

AN
INAUGURAL
EXPERIMENTAL INQUIRY, &c.

SECTION I.

Obſervations on Digeſtion.

MAN, like every other being in nature, requires a continual and regular ſupply of food, for the ſeveral purpoſes of ſupporting life, of promoting the growth of his ſyſtem until he arrives at maturity, and of forming new parts when ſuch become neceſſary. Indeed, on contemplating the complicated ſtructure of his frame, it is evident, that a continual loſs of the ſolid and fluid parts, of which he is compoſed, muſt be the inevitable effect of every action or function of life which he performs.

Nature, always wiſe, to obviate this general waſte of his ſyſtem and continual tendency to decay, made it neceſſary that he ſhould have ſome inducements to repair it, and thus be reminded of the connection which exiſts between aliment and life. Accordingly we find he is indued with the ſtimulus of hunger and thirſt, which, together with the pleaſure

ſure he receives from gratifying thoſe appetites, induce him to take into his ſtomach a certain quantity of matter to allay thoſe diſagreeable ſenſations. This matter includes, not only the ſeveral kinds of animal and vegetable ſubſtances which we denominate food, but alſo comprehends the fluids taken in with which they are diluted.

Theſe having arrived at the ſtomach which is the great receptacle of his aliment, a greater or leſs length of time is requiſite, according to circumſtances, for them to undergo thoſe proceſſes which are eſſential to their aſſimilation being completed. In fact, I conceive it an impoſſibility to determine with any certainty, the exact time neceſſary for the digeſtive organs to perform their reſpective functions, as that will depend in a great meaſure on the ſtrength of thoſe organs, on the quality, quantity and manner in which the food is prepared, its previous maſtication, and various other cauſes of which we are not always aware. The moſt prevalent opinion on this ſubject, is, that from about the third to the ſixth hour after food is taken, it is diſcharged, through the pylorus, of a pultaceous conſiſtence. There are however ſome extraordinary deviations from this allotted time, which cannot but convince us of the great uncertainty of calculations of this nature. We have on record inſtances related of ſubſtances remaining in the ſtomach indigeſted for months; and on the contrary, that in two hours after food was taken into the ſtomach, that organ was found empty.* Theſe I conceive to be rare occurrences, neither do I believe it by any means common, even in the ſpace of three hours, for the ſtomach to diſcharge its contents; as in a majority of mankind, I preſume, a much longer time is requiſite for food to undergo thoſe changes, which are uſually effected on it during its ſtay in that organ.

To our ſeveral kinds of aliment, different condiments are added by various nations, many of which merely gratify the palate, while others aſſiſt in promoting digeſtion. Among us at preſent ſea ſalt is moſt univerſally uſed for this purpoſe.† Profeſſor Chaptal tells us, that the acidulous tartrite of pot-aſh is greatly conſumed in the north of Europe, where it is uſed as a table ſeaſoner; and Profeſſor Barton has informed me, that the Creek Indians make uſe of hiccory aſhes, and that in ſome of the ſouthern ſtates, the aſhes of a particular kind of marſh-graſs were formerly preferred for the ſame purpoſe.

Beſides this difference in nations with reſpect to condiment, there is one of ſtill more conſequence, which is, their ſtriking peculiarities in the choice of food. We find, that in Lapland, Iceland,

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* Vid. Haller's Element. of Phyſiol. Tom. VI. page 281.

† According to the experiments of the celebrated Pringle, a ſmall quantity of ſea ſalt haſtens putrefaction, while a larger quantity retards that proceſs. *Diſeaſes of the army, Apendix, paper 3. Exper. 25.*

Greenland, Norway, and other cold countries, the inhabitants live chiefly on fiſh and fleſh;[*] while, on the contrary, we are informed of certain ſects in India, who live almoſt, if not ſolely, on a vegetable diet. Both the Laplander and the Indian enjoy their health in theſe extremes; their habits, together with their climates, being better adapted to their reſpective modes of living; as the ſouthern latitude, in which the latter reſides, appears to prevent his ſubſiſting for any length of time, on fiſh or fleſh; while its ſtimulus is abſolutely neceſſary to ſupport the general waſte of the ſyſtem, to which the former is expoſed from cold. A majority of mankind live in the medium of theſe extremes; experience having taught them, that a due proportion of animal and vegetable food, is the better adapted for their nouriſhment, the one counteracting the ill effects ariſing from the other.

Of all animals man appears to be the moſt omnivorous. Deſtined to range through every the moſt diſtant part of our globe, he is capable of accommodating himſelf to the food of every country. Certain other animals are likewiſe capable not only of changing their accuſtomed diet, but ſometimes acquire ſo vitiated a taſte, as to refuſe their former food. This was particularly the caſe with the wood pigeon of Spallanzani. This acute phyſiologiſt tells us, that by dint of hunger he brought this bird to reliſh fleſh ſo well, that it refuſed every other kind of ſuſtenance, even grain, of which it is naturally ſo greedy. Various other facts of a ſimilar nature, are found on record. Von Troil informs us, that the Icelanders, when there is a ſcarcity of fodder, feed their cattle with *ſteenbitr*, (a kind of fiſh) which, together with the heads and bones of cod, is beaten ſmall, and mixed with one quarter of chopped hay. He further adds, that the cattle are fond of this food, and yield a good deal of milk after having uſed it.*

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[*] This is ſometimes (though more rarely) the caſe in ſouthern latitudes. At Orange river, in Africa, Fordyce tells us, that the inhabitants live upon limpets, dead and putrid ſeals and whales, not taſting a particle of vegetable food of any kind, excepting aromatics.

Treatiſe on Food. p. 175.

Profeſſor Barton, in his courſe on natural hiſtory, has likewiſe related a fact no leſs intereſting. He has told us, that deer have been frequently obſerved to feed on dead fiſh, which had been waſhed up on the banks of the Suſquehannah and other rivers. Theſe are inſtances ſufficient for my purpoſe, but many others of equal importance might be collected.

From the ſeveral kinds of aliment taken into the ſtomach, man is plentifully ſupplied with fluids, and from the component parts of theſe fluids, is the growth of his ſyſtem and the ſolids of his body produced. This growth of his ſyſtem and production of ſolids is induced, although he may ſubſiſt on very different kinds of food, as by the peculiar operation of his digeſtive organs, he is capable of aſſimilating, by certain proceſſes, matters taken from either the animal or vegetable kingdoms into a fluid *ſui generis.* Theſe proceſſes of aſſimilation are comprehended in the term digeſtion; by it we are to underſtand, thoſe proceſſes which take place in the digeſtive organs of man, and by which his food is converted into laudable chyle.

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* Vid. Letters on Iceland, p. 133.

The *modus operandi* of nature in this converſion of our food into chyle, has attracted the attention of philoſophers in every age, and various theories have been advanced to explain the phenomena which occur. With this view we find, that, the theories of *the heat of the ſtomach*, of *mechanical action*, of *fermentation*, of *ſolution* and others, have all been advocated by men, whoſe fame has added reputation to their opinions. But, as no one of theſe can excluſively account for all the phenomena of digeſtion, and as in the choice of a theory, the preference ſhould always be given to ſuch a one, as will explain to us the moſt of them; I am induced to adopt another, and attempt to prove the dependency of this important function, on the united cauſes of ſolution and fermentation.

Of the Heat of the Stomach.

The theory, of the heat of the ſtomach, was at one time ſo faſhionable, that Profeſſor Blumenback tells us, inſtead of the term digeſtion, that of coction, was formerly uſed by the greater part of phyſiologiſts.* This opinion, however, I believe at preſent has but few advocates, as I preſume no perſon will now contend that heat is the ſole cauſe of digeſtion. This would be equally as incorrect to ſay, that it does not aſſiſt in promoting that proceſs. While we refuſe to admit, that heat is the ſole efficient cauſe of digeſtion, we cannot but acknowledge its effect in expediting that proceſs, as it has been long ſince made evident by experiment. It therefore only remains that we ſhould ſhew from the ſituation of the ſtomach, that it is advantageouſly ſeated to be ſupplied with heat from its neighbouring parts, as we may eaſily conceive from contemplating their relative ſituations with reſpect to it. We find that its right ſide is covered by the thin edge of the liver; its left touches the ſpleen; that behind it is the ſeat of the pancreas, and immediately above it is the diaphragm; that the peritoneum lies before it, which, by the action of the abdominal muſcles, gives it a motion diametrically oppoſite to that which it receives from the diaphragm in reſpiration; and that the aorta, the largeſt artery in the body, lies juſt behind it. All of theſe circumſtances muſt tend to give it additional heat. Hence we may with propriety acknowledge the accuracy of Dr. Barry, when he tells us, that "the heat of

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* Inſtitut. Phyſiol. Vol. II. p. 23.

" the ſtomach in a healthy man, is greater than the " common heat of ſun in a ſummer's day."*

Of Mechanical Action.

To diſprove that the mechanical action of the ſtomach has any effect in promoting digeſtion, I need ſay but little. Facts ſpeak for themſelves. That accurate experimenter, the Abbe Spallanzani, has decidedly proven the very wonderful muſcular action exerted in the ſtomachs of ſome animals; but his experiments likewiſe tend to ſhew, that no ſuch action takes place in the human ſtomach. Having frequently ſwallowed wooden tubes during his experiments, which were made ſo thin as to be incapable of bearing the ſlighteſt preſſure; he never, in a ſingle inſtance, diſcovered one of them to be broken. In addition to this, he mentions the fact of cherries and grapes being voided entire, as I have myſelf frequently obſerved. He likewiſe relates an experiment with ripe grapes, which we all know to be incapable of bearing the leaſt mechanical action, which appears to be directly in point. " Of twenty-five," ſays he, " which I " ſwallowed, eighteen were voided entire, of the " other ſeven, the ſkins only appeared."† Theſe experiments alone, I deem ſufficient to prove, that no triturating power is poſſeſſed by the human ſtomach,

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* Vid. Treat. on Digeſt. p. 8.

† Natural Hiſtory, Vol. I. p. 222.

mach, particularly as the fact of grapes being voided entire, muſt be notorious to every perſon who has attended to the ſubject. In ſhort, I do believe, that the muſcular fibres of the ſtomach, have no other effect on our food, than merely that of propelling it through the pylorus.

Of Fermentation.

While ſome phyſiologiſts of conſiderable reputation, have conſidered fermentation as quite ſufficient to explain all the various phenomena of digeſtion, others of equal celebrity have contended, that no ſuch proceſs takes place in a healthy ſtomach. This diverſity of opinion, I cannot attribute to motives of prepoſſeſſion in favour of any particular theory, but would rather preſume, it was the conſequence of a ſuppoſition, that to admit the one to be a fact, would be a tacit acknowledgment that the other could not be true. This too I ſuppoſe is the reaſon, why even at the preſent time, thoſe who have aſcertained the ſolvent power of the gaſtric juice, will not admit that fermentation ever takes place in a healthy digeſtion. But this, perhaps though too common an error, is ſtill one by no means the leſs prominent. Does not chemiſtry teach us, that nature frequently requires in her operations a multiplicity of cauſes to induce a ſingle effect? Why then, if one of the cauſes, which have been advanced, is not ſufficient to explain the phenomena which occur, ſhould we attempt the eſtabliſhment of another, equally inadequate to account for the wonderful effect of the converſion of our food into chyle?

The operations of nature are uniform, and frequently too deep for the ſhallow limits of human wiſdom to demonſtrate; but I think, when we ſhall have been more ſucceſsful in our experiments, it will be found, that digeſtion depends on the combination of ſeveral cauſes, and that fermentation does certainly take place, as I ſhall endeavour to prove in a ſubſequent part of this eſſay.

Of Solution.

That ſolution does likewiſe take place in digeſtion, I do believe, as the experiments of many phyſiologiſts of conſiderable reputation tend to prove the poſition, and my own have induced me to embrace it. The opinion, however, is by no means novel. Barry informs us, that "Baſil Valentine was the "firſt among the chemiſts who ſuppoſed that animal digeſtion was owing to an acid diſſolving "menſtruum." That "Paracelſus received this "opinion from him." And that "Van Helmont "carried it farther; and aſſerted, that the ſpleen "prepared this menſtruum, which was from thence "conveyed into the ſtomach, by the *vaſa brevia.*" Hence it appears, that the theory of ſolution has been long ſince favoured. Since when, very accurate experimenters have written, in hopes completely to decide the queſtion in its favour. But, although they have moſt demonſtratively proven the ſolvent power of the gaſtric juice, they have by no means ſhewn that fermentation does not likewiſe take place. For my own part, I do not heſitate in believing, that both ſolution and fermentation do take place in a healthy digeſtion; indeed I think, with correctneſs I may venture to aſſert, that in the human ſtomach, fermentation does as neceſſarily follow ſolution in the converſion of food into chyle, as thought ſucceeds impreſſion in the formation of ideas.

Food, in the firſt inſtance, is conſiderably attenuated, by the maſtication which the rotatory motion of our jaws and preſſure between our molares are ſo capable of giving it. Its particles being thus divided, are intimately mixed with the mucus of the mouth and ſaliva, after which it paſſes down into the ſtomach. This we may term a proceſs preparatory to digeſtion. It is certainly one of much more conſequence to the perfect digeſtion of our aliment, than is generally ſuppoſed; as it is evident thoſe perſons, who half chew or bolt their victuals, as it is called, are generally ſubject to all the numerous diſeaſes ariſing from indigeſtion. Hence appears the neceſſity of perſons being particular in the maſtication of their food, as nothing ſcarcely can be of more injury to the conſtitution, than continually to ſupply the ſtomach with indigeſtible half-maſticated food.

Something, in its effect very ſimilar to maſtication, is obſervable in domeſtic fowls. They, by a peculiar inſtinct, take into their gizzards, pebbles and gravel, which certainly ſerves in them every purpoſe, which teeth do in ſome other animals. I have made uſe of the word inſtinct through choice, becauſe I cannot believe with Spallanzani, that they are picked up by mere accident, or through their ignorance in miſtaking them for food. One fact appears to oppoſe his theory, which is, that thoſe fowls which are kept on a gravelly ſoil are rarely if ever found to have a greater number of ſtones in their gizzards, than thoſe raiſed where leſs gravel is preſent. Again, if they were picked up by accident, we ſhould expect that they are not at all neceſſary to their health; whereas, the very reverſe of this is the fact. A very reſpectable author, who inveſtigated this ſubject with conſiderable ſucceſs, by experimenting on chickens hatched with artificial heat, has given us the very beſt information I have peruſed. He ſays, "I have hatched vaſt "numbers, and frequently have given the chickens "ſmall ſeeds whole, taking care that they ſhould "have no ſtones. In this caſe the ſeed was hardly "digeſted, and many of the chickens died. With "the ſame treatment in every reſpect, others who "had their ſeeds ground, or have been allowed to "pick up ſtones, have none of them been loſt."*

This would appear to ſhew, that pebbles are eſſentially requiſite to the healthy digeſtion of theſe animals. Indeed, the experience of many perſons tends to prove this to be the caſe, as we often hear of their ſending for gravel for their poultry, and when interrogated why they do this, they tell us, that without it their fowls grow poor, dwindle away, and ſometimes die. Mr. John Hunter, commenting on this aſſertion of Spallanzani, that pebbles are picked up by birds through chance or ignorance, ſays with much humour that "it appears "ſingular, that only thoſe which have gizzards "ſhould be ſo ſtupid."

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* Fordyce on Food, p. 24.

The more freely food is maſticated, and the more minutely it is divided; the leſs heavy does it lay on the ſtomach, and the more eaſy it is of digeſtion. The experiments of Spallanzani,* made on himſelf, prove the latter poſition; the former is obvious to common obſervation. Maſtication not only promotes digeſtion, by minutely dividing the matters to be carried into the ſtomach; but likewiſe, by mixing them with ſaliva to form a pulpy maſs, it involves a very conſiderable portion of atmoſpherical air, which, being rarefied by the heat of that organ, tends conſiderably to burſt the ſeveral particles of food from each other. Here then we ſee that it indirectly promotes ſolution, for, as the ſeveral particles of food are ſeparated from each other, in that proportion will ſolution be expedited; becauſe, a greater number of ſurfaces will thereby be expoſed to the action of the gaſtric juice, and of conſequence the food will be the more ſpeedily diſſolved.

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* Natural Hiſtory. Vol. I. p. 224.

In the inveſtigation of a ſubject like the preſent, it is neceſſary that we attend particularly to the cauſes which influence digeſtion, and judge of their effects, by comparing them to the effects of ſimilar cauſes out of the body. And again, by accurately examining the products of digeſtion, and by comparing them to ſimilar products out of the body, thereby inveſtigate its cauſes. Thus, as all vegetable ſubſtances, capable of undergoing the vinous fermentation, contain the conſtituent principles of carbonic acid and of alcohol, and as theſe ſubſtances are obtained from ſuch vegetables, the former during the fermentative proceſs, and the latter after that proceſs has ceaſed; it is evident, if ſuch products are obtained from the digeſtion of food in the ſtomach, it muſt be the effect of a ſimilar proceſs. The ſame obſervations will apply to the other ſtages of fermentation.

In the following pages, I ſhall endeavour to relate in brief detail, ſuch facts and experiments as have induced me to adopt the preſent theory. This will be done in three ſeveral ſections. In the firſt I ſhall proceed to deliver a few obſervations on the matters which influence digeſtion. The ſecond will contain an attempt to ſhew, how far ſolution is admiſſible in digeſtion. And in the third and laſt, I ſhall endeavour to relate, why it is preſumable that fermentation does likewiſe take place.

SECTION II.

OF THE MATTERS WHICH INFLUENCE DIGESTION.

THE chief of theſe are, ſaliva, the gaſtric juice, the mucus of the primæ viæ, the bile, and the pancreatic juice. And;

1ſt. *Of the Saliva.*

By ſaliva we mean that colourleſs, glutinous, and reſolvent fluid, which is ſecreted by the parotid, ſubmaxillary, and ſublingual glands. It is perfectly taſteleſs and inodorous in health, and conſiſts of water holding a mucilage and certain ſalts in ſolution.

On being placed on the fire, its watery part ſpeedily evaporates, leaving its ſalts in a ſtate of cryſtallization, reſembling two or three different kinds of ſalt. Theſe are, according to Fordyce, ſea-ſalt and ſal-ammoniac, together with various other cryſtallizations of irregular forms; and agreeable to Plenck, ſal-ammoniac and animal earth; the former he inferred from triturating quicklime with ſaliva; the latter from ſalival calculus and the products of fire.

Saliva from its ſapid nature, gives an increaſed flavour to food. It tends to relieve thirſt, by ſupplying the mouth and fauces with a ſufficient quantity of moiſture; and has a certain and evident effect in digeſtion. It does not however, poſſeſs a ſolvent power, as has been aſſerted by ſome, at leaſt it does not poſſeſs this property in a greater degree than ſimple water.

I put an ounce of pure ſaliva, and as much of ſimple water, into two ſeparate phials; to each of theſe I added two drachms of roaſted veal. Theſe phials were placed uncorked in a ſand bath, which was kept as near as poſſible to the heat of the human body. After ſuffering them to remain at reſt for eight and forty hours, I decanted the water and ſaliva from each of the phials. The veal which had been immerſed in water, and that which had been in ſaliva, were now placed ſeparately on bibulous paper, and as much of their moiſture removed as poſſible. The one which had been in water, was now weighed; it had loſt twenty-three grains of its weight. On weighing the other, it had loſt no more. This would certainly ſeem to ſhew, that ſaliva has no ſolvent power, at leaſt not out of the body.

Again, it has been aſſerted, that ſaliva corrodes copper and iron more ſpeedily than ſimple water.* To aſcertain this point, I made the following experiment:

Having placed twelve grains of ſheet copper in a phial containing ſaliva, and the ſame quantity in another of equal ſize, containing water only; I put them both in a ſand bath, which was generally about the temperature of one hundred of Fahrenheit.

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* Vid. Haller's Element. Phyſiol. Tom. VI. p. 54. and Plenck on Hum. Fluids, p. 72.

heit. They were both kept in this ſituation for one week, at the expiration of which time, they were taken out, wiped dry, and weighed. I could not diſcover, that either one of them had loſt more of its weight than the other. From this I preſume we may conclude, that ſaliva has not a much greater effect, if any, in the corroſion of copper, than ſimple water.

Deſirous of knowing what effect ſaliva has in digeſtion, and with a view of aſcertaining whether it poſſeſſed a fermentative property or not, I expoſed equal portions of leavened bread and roaſted veal, in two ſeparate flaſks, to the heat of a ſand bath, which I was careful to keep as near as poſſible to the temperature of the human body, or from ninety-ſix to ninety-eight of Fahrenheit. To one of theſe flaſks, I had previouſly added eight ounces of water; to the other, ſeven ounces of water and one of ſaliva. The veal, to which ſaliva was added, I took the precaution of maſticating, in order that it might be the more accurately blended with that fluid. In the ſpace of ſix hours, the one containing ſaliva ſmelled a little ſour, emitted air bubbles, and ſhewed evident marks of fermentation. Whereas, that proceſs did not commence in the other, which contained water alone, until four hours after.

Profeſſor Ruſh has long ſince made a ſimilar and very decided experiment. "To elucidate the pro-
" perties of ſaliva," ſays our author, " I placed
" mutton

" mutton and bread, of each two drachms, in two " glaſs veſſels. To one I added an ounce and a " half of ſaliva, to the other the ſame quantity of " water, and placed both of them in a ſand bath. " Five hours having elapſed, the mixture with ſa- " liva began to ferment. In ſeven hours, it diſ- " covered evident marks of acidity; whilſt, in " that, to which water was added, ſcarcely any " motion was perceptible. After twelve hours had " elapſed, the mixture with ſaliva emitted a putrid " ſmell; whilſt the mixture with water remained " mild and inodorous to the twentieth hour."[†] An experiment ſomewhat ſimilar, has likewiſe been made by Pringle, but not with exactly the ſame ſucceſs. He expoſed two drachms of freſh meat and the ſame quantity of bread, together with water and ſaliva, to the heat of a furnace, kept at the temperature of 100 of Fahrenheit. The mixture remained about two days, he ſays, with ſcarce any viſible fermentation; but on the third day that proceſs became manifeſt.[‡] This inveſtigation, however, does not appear to have been made ſufficiently fair, for he tells us, that his experiment was made in a cloſed phial. Now we know that the preſence of vital air, is a circumſtance eſſential to fermentation, and hence its excluſion muſt have been the cauſe of that proceſs being retarded. In every comparative experiment like this, every circumſtance ſhould ſurely be made as ſimilar as poſſible, to thoſe which occur in the living ſtomach, and of conſequence there ſhould be an admiſſion of vital air, as this fluid is always involved in ſaliva, which is ſeveral times in the courſe of a ſingle minute conveyed into the ſtomach.

[†] Inaug. Differ. de Coctio. Cib. in Vent. Exper. V.

[‡] Vid. Diſeaſes of the Army, Appendix, paper 4, Exper. 30.

We are informed, that ſome uncivilized nations, are ſo well aware of the fermentative property of ſaliva, that they prepare an intoxicating drink, by mixing ſaliva with certain vegetable ſubſtances. Plenck relates, that they prepare it from the chewed roots of the *jatropha manihot* or *caſſada* and *piper methiſticum.*

Whether ſaliva poſſeſſes a ſeptic or antiſeptic quality, has likewiſe been a ſubject of ſome controverſy. To ſatisfy myſelf on this head, I placed equal quantities of roaſted veal, in two ſeparate phials of the ſame ſize. To one, I added an ounce of pure ſaliva, to the other, as much of ſimple water; theſe I placed, uncorked, in a ſand bath, which I endeavoured to keep as near as poſſible to ninety-ſix of Fahrenheit. The heat of the bath, however, was ſometimes as high as one hundred and ten, but never below fifty. In twenty-two hours, the one containing ſaliva emitted a putrid ſmell; the other only ſmelled ſour. In eighteen hours after, I examined the phials again; they both ſmelled putrid; the one with ſaliva was the moſt ſo, and likewiſe had changed its colour a little, which was not the caſe with the other.

From theſe experiments, I preſume it appears, that ſaliva poſſeſſes the property of promoting both fermentation and putrefaction; and not that it promotes fermentation, and at the ſame time retards putrefaction, as has been ſuppoſed.

The quantity of twelve ounces of ſaliva, is generally ſuppoſed to be ſecreted by an healthy adult, in the ſpace of twenty-four hours. At leaſt this is according to the experiment of Nuck, whoſe aſſertion I believe continues to be conſidered as the moſt correct. The quantity however, is capable of being wonderfully increaſed by certain medicines and other ſtimulants. Indeed Haller ſpeaks of fifteen ounces being effuſed in the ſpace of thirty minutes.*

2d. *Of the Gaſtric Juice.*

The gaſtric juice in health, is a colourleſs and turbid fluid, void of both taſte and ſmell; and I muſt add, bearing conſiderable reſemblance to ſaliva. I do not believe, as ſome authors have ſuppoſed, that it is ſecreted from the ſmall glands of the ſtomach, as it is inconceivable to me, how ſuch minute glands could be the ſource of ſuch large quantities of this fluid, as are at times ſecreted. I therefore ſhall prefer embracing the theory of thoſe, who ſuppoſe the immediate ſource of the gaſtric juice to be the extremities of the arteries of the ſtomach; for I can as eaſily conceive, in the wiſdom of nature, that arteries may be expanded on the coats of the ſtomach in ſuch manner as to perform ſecretions, as that they ſhould be united together by the cellular membrane, in the form of glands for the ſame purpoſe.

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* Vid. Element. Phiſiolog. Tom. VI. p. 59.

Many perſons are at preſent of opinion, that the gaſtric juice contains an acid. From the experiments of the Abbe Spallanzani, from thoſe of his colleague Profeſſor Scopoli, and indeed from thoſe of many others, I think we have good reaſon to doubt of the preſence of a ſenſible acid in the gaſtric juice.

Having obtained ſome pure gaſtric juice from my own ſtomach, I found it capable of coagulating milk very readily. This however I conceive as no proof of acidity being preſent, as I have coagulated it with a ſolution of freſh runnet, in which I could not detect the ſmalleſt particle of acid. Nothing can be more erroneous than the opinion which prevails among ſome perſons, that acids alone have the power of curdling milk. The truth is the very reverſe of this; for it is now known, that the heart, lungs, and even liver of a turkey have been diſcovered to poſſeſs this property.* It will ſurely not be ſaid that they are likewiſe acid. Again, certain vegetables, as the galium luteum, or ladies bed-ſtraw, vaillantia cruciata or croſs wort, rubia tinctorum or madder, carduus or thiſtle, cynara ſcolymus or artichoke, as well as many others, have been obſerved to have this effect. Indeed living fiſh have been obſerved to have this property; and Jacquin tells us,[*] that even lime-water produces an imperfect coagulation of milk.

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* Vid. Spallanzani. Nat. Hiſt. Vol. I. p. 271.

Another fact, of no trivial import in the deciſion of this queſtion, is related by Mr. John Hunter. This gentleman tells us, that "in the ſlink calf, "near the full time, there is no acid found in "the ſtomach; although the contents have the "ſame coagulting powers with thoſe of animals "who have ſucked."[†] Now, as this coagulating property is evidently communicated to the ſtomach by the gaſtric juice, and as an acid could not be detected in the ſtomachs of theſe young animals, although they poſſeſſed this coagulating property, I think it appears ſufficiently clear, that an acid does not exiſt naturally in the gaſtric juice. This fact I conſider as concluſive, at leaſt in as far as it prevents our being deceived by acids which are evolved in digeſtion.

Haller likewiſe appears to have been fully perſuaded, that pure gaſtric juice does not contain an acid, and has quoted the authority of at leaſt a dozen perſons to prove his aſſertion.[‡] To determine this point however more ſatisfactorily, as it is of ſuch importance in the adoption of a theory of digeſtion, I made the following experiment.

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[*] Vid. Element. Chem. treat. de lacte.

[†] Vid. obſervations on the animal. œconom. p. 163.

[‡] Vid. Element. phyſiol. Tom. VI. p. 143.

Deeming it neceſſary to obtain the gaſtric juice perfectly free from any extraneous matter, and likewiſe from any acidity of a former digeſtion, I kept a cat faſting eight and forty hours, after which it was killed. I found no food in its ſtomach, and but a ſmall quantity of gaſtric juice. This I ſubmitted to the uſual teſts for detecting the preſence of an acid, but could not diſcover any. Hence I have been induced to conclude, that the gaſtric juice does not contain a ſenſible acid, and that whenever an acid is preſent in that fluid, it muſt either be the effect of diſeaſe or proceed from the remains of ſome former food.

Barry ſays, "that the humours which are contained in the ſtomachs of the moſt rapacious birds, "fiſhes, and beaſts of prey, have never an acid, "but a ſaline taſte."* And from the chemical analyſis of the gaſtric juice of the crow, by Profeſſor Scopoli, we find he could not detect the preſence of an acid in that fluid, but diſcovered it to be compoſed of, firſt, pure water; ſecondly, a ſaponaceous and gelatinous animal ſubſtance; thirdly, ſal-ammoniac; and fourthly, an earthy matter ſimilar, he ſays, to that which exiſts in all animal fluids.

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* Treat. on Digeſtion. p. 22.

We come next to conſider the ſolvent power of the gaſtric juice, which is the great baſis on which the whole of the preſent favourite theory of digeſtion depends.

Spallanzani tells us, that this fluid in a dog, not only acts on bone, but even corroded the denſe enamel of two dentes inciſores taken from the upper jaw of a ſheep.* And from experimenting on himſelf, he found he could digeſt not only "muſ-"cular fibres and membrane, but tendon, carti-"lage, and even bone itſelf, when not too hard." From the excellent inaugural theſis of Dr. Stevens, we likewiſe learn, that various animal and vegetable ſubſtances, were diſſolved by the gaſtric juice of the human ſtomach when incloſed in ſpheres. In like manner bone, and even ivory ſpheres were diſſolved, which he had introduced into the ſtomach of a dog.

Since the publications of theſe two ingenious gentlemen, and particularly ſince the paper of Mr. John Hunter has appeared, further inveſtigation of the ſolvent power of the gaſtric juice ſeemed unneceſſary. This gentleman diſcovered, that in thoſe perſons who had died of violent deaths, the ſtomach itſelf was corroded and diſſolved at its great extremity, and accounts for the ſtomach not being acted upon during life, on the theory of a *living principle.* Several facts tend to ſhew, that there is ſomething innate in living matter, which reſiſts the action of the gaſtric fluid. It is well known, for inſtance, that worms exiſt in the human ſtomach unaffected by digeſtion while living, but are ſpeedily acted upon by the gaſtric juice as ſoon as they are deprived of life. To account for this fact, we muſt conſider the gaſtric juice as a mere chemical ſolvent; then, by reflecting that all chemical ſolvents act by attraction, we may ſay, that the action of the veſſels in living matter, is too great to be overcome by the attractive force of the gaſtric juice, and conſequently their combination cannot take place. Another fact which ſhews, that the ſolvent power of the gaſtric juice is decidedly inert, as it reſpects ſome kinds of living matter, is, that there are ſeveral ſpecies of ſerpents, and particularly the rattle ſnake, who receive their young on the leaſt alarm, down their throats into their ſtomachs, where they have been known to remain for three and four hours. Fifteen or twenty of theſe young rattle ſnakes, have been found in the ſtomach of an old one, and not in the leaſt injured by the gaſtric juice.* "A polype" too "inſerted into the ſtomach of another polype, continues to live as before."†

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* Vid. Natural Hiſtory, page 211.

Although convinced from theſe facts, of the inertneſs of the gaſtric juice on theſe animals, ſtill I was not ſatisfied of its action on other living animals. I therefore determined on an experiment with the gaſtric juice of a dog. For this purpoſe I kept a dog faſting twenty-four hours, at the expiration of which time I obliged him to ſwallow a number of ſmall ſpunges. As his ſtomach retained theſe very readily and without any apparent inconvenience, I ſuffered him to remain at reſt for three hours, immediately after which he was killed. From theſe ſpunges I obtained about half an ounce of very pure gaſtric juice. In it I immerſed a common earth-worm. The animal writhed about, and ſhewed ſymptoms of great diſtreſs. I corked the phial, and being at ſome diſtance from home, walked with it in my pocket to my reſidence. At this time I examined it again; exactly half an hour had elapſed from the time of my placing it in the phial. The animal ſhewed no ſymptom of life, and on taking it out of the phial, I found on its body evident marks of violent inflammation. Suſpecting this, however, to be the conſequence of the heat of my pocket, which (as it was in the month of April) I believe to have been about 80 of Fahrenheit, I repeated the experiment. The animal after being immerſed in the gaſtric juice, at the temperature of 70 of Fahrenheit, for fifty minutes, ſhewed no ſigns of life; but there was no inflammation evident, as in the former. Knowing, however, that theſe animals live in a temperature ſtill lower than this, I determined again to repeat the experiment. To effect this I placed an earth-worm in the phial of gaſtric juice as before, and covered it over with the ſame ſod and in the ſame ſpot from which the animal was taken. Fahrenheit's thermometer ſtood in the turf at 60. In an hour I examined the phial, took out the worm, and found it lively and not in the leaſt injured; but on increaſing the temperature to 70, it died as the laſt. Hence we may infer, that this animal alſo reſiſts the action of the gaſtric juice while living and at its own temperature. How far this will be found to be the caſe with other animals, I am at preſent unprepared to decide. I am aware that, according to the experiments of Dr. Stevens, a living leech, which ſome perſons have ſuppoſed to be an animal deſtitute of pores and capable of ſuſtaining a degree of heat equal to the human temperature, is digeſted by the human ſtomach.* But, that they are capable of ſuſtaining this degree of heat, I am not ſatisfied; at leaſt, of this we may be certain, that the temperature, at which they uſually live, is not by any means as high. I had rather preſume, that as in my experiments, the animal in this inſtance, had died previous to its being acted on by the gaſtric juice. Plot does indeed tell us, that he has ſeen the eyes of a carp and the noſe of a roach, which were taken out of a jack-fiſh, digeſted, while they were yet alive.† And Cornelius is likewiſe ſaid to have found a ſnake half digeſted in a bird's ſtomach, while life in that animal was ſtill perceptible.

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*Profeſſor Barton's Lectures on Natural Hiſtory.

† Spallanzani's Natural Hiſtory, Vol. I. p. 111.

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* Vid. his Inaug. Diſſer. pub. at Edin. in 1777.

† Vid. Plot's Staffordſhire. Ch. 7. Sect. 37.

In all experiments with a view to aſcertain the action of the gaſtric juice on different kinds of living animals, to proceed fairly I think we ſhould keep this fluid exactly at the natural temperature of the animal immerſed in it, otherwiſe it may be deſtroyed, either by an exceſs, or too great abſtraction of heat. The gaſtric juice of different animals too, requires as different temperatures to promote their action. Thus we learn that this fluid in amphibious animals and in fiſhes, acts on aliment at the common temperature of the atmoſphere; but in moſt animals of the claſs mammalia, a higher temperature is requiſite. The gaſtric juice of different animals varies in another reſpect ſtill more conſiderably. Thus, in ſome animals, we ſee that it appears deſtined to act on animal food only; in others, on vegetables alone; and in others again, on both.

The gaſtric juice of man acts on antimony and copper; the former I relate on the authority of Profeſſer Chaptal,† the latter on that of Profeſſor Barton. This laſt gentleman mentions, in his lectures, the caſe of a child, who was much indiſpoſed and ſalivated, in conſequence of ſwallowing a cent; the cent when voided, was examined and found to be ſenſibly diminiſhed.

According to Jacquin, Spallanzani and others, the gaſtric juice of itſelf has little tendency to either fermentation or putrefaction; but when mixed with other ſubſtances, its effect is rather to retard thoſe proceſſes.

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† Vid. Elemen. Chem. Vol. II. p. 260.

From the numberleſs ſmall arteries of the ſtomach, we may preſume the quantity of gaſtric juice ſecreted to be very great; but like other ſecretions it is increaſed in quantity by ſtimuli, and particularly by the ſtimulus of food.

3d. *Of the Mucus of the Primæ Viæ.*

This mucus is found on the internal ſurface of the ſtomach and inteſtines, and covers it very completely. It has the appearance, conſiſtence, and properties of mucus in other parts of the body. It is ſecreted from ſmall glands, which are ſituated under the villous coat of the primæ viæ. The quantity ſecreted appears to be very great.

Its uſe ſeems to be that of lubricating the ſurface of the primæ viæ, in order to facilitate the paſſage of their contents. It muſt likewiſe defend the internal ſurface of the ſtomach and inteſtines, from the action of the gaſtric juice, and from the acritude of bile when regurgitated.

To aſcertain whether this mucus has any effect, either in promoting or protracting the proceſs of fermentation out of the body, I placed equal portions of leavened bread in two flaſks, each containing eight ounces of water. Reſerving one of theſe as a criterion, I added to the other about four drachms of this mucus, which I had procured from the ſtomach of a ſubject, who had died of a fall. Both flaſks were placed in a ſand bath, which was kept as nearly as poſſible at the temperature of the human body. In ſeven hours, the one containing mucus emitted air bubbles and ſmelled ſour, whereas the one without mucus had no appearance of fermentation for ſeveral hours after.

4th. *Of the Bile.*

The name of bile, has been uniformly given to a peculiar fluid, excluſively found in animal bodies, and which is ſecreted from a gland of a ſingular ſtructure, called the liver. It is more or leſs of a yellowiſh-green colour, of a diſagreeable bitter taſte, of a thicker conſiſtence and more plaſtic than ſaliva, of a ſingular aromatic ſmell when evaporated, which has been compared to that of muſk, and which is by ſome thought agreeable.

Bile differs from all other ſecreted fluids in the body, in this, that it is not like them, ſeparated from florid arterial blood, but is ſecreted from the dark coloured blood of the vena portarum, which is nothing more than a large veſſel, made up by the concurrence of all the veins of the viſcera of the abdomen, (except thoſe of the liver,) which empty their blood into it for the purpoſe of ſecretion. Now a plentiful ſupply of blood to a gland, being eſſential to the ſecretion of a fluid, and this blood being conveyed to the liver by the vena portarum, it has, and I think with propriety, been ſaid, to perform the office of an artery. It ramifies every where throughout the ſubſtance of the liver, and terminates in two very different kinds of veſſels. The one returns the blood, which is no longer fit for ſecretion, again into the general circulation; the others are the ſecretory veſſels. In the former caſe, the extremities of the vena portarum inoſculate with thoſe of the hepatic veins, and thus the blood is returned to the inferior or aſcending cava. In the latter, the ſecreting veſſels ſoon terminate in the pori biliarii, by the union of which, in their paſſage out of the liver, trunks of a larger ſize are gradually formed, till at length they terminate in one of conſiderable magnitude, known by the name of the hepatic duct. This duct in its turn, ſoon terminates in another, which has gotten the name of the ductus communis choledochus, in conſequence of its being the common duct of the liver and gall-bladder, through which bile is continually diſtilling into the duodenum, in which inteſtine this duct ends. Juſt however where the hepatic duct ends, and where the ductus communis choledochus begins, another duct ariſes, which extends to the gall-bladder, from whence it has received the name of ductus cyſticus.

The ſecretion of bile, is a ſubject as yet involved in conſiderable obſcurity. While ſome phyſiologiſts have conſidered it as the effect of a peculiar ſecretion in the liver, others of equal reputation, have aſſerted, that it is found formally in the blood. The correctneſs of theſe aſſertions are only to be determined by experiment, as therefore I cannot do this at preſent, I ſhall not venture a conjecture on the ſubject.

The bile being once ſecreted, it is received by the ſmall branches of the hepatic duct called pori biliarii, from whence it paſſes into larger branches, till at length it gradually arrives at the great trunk of the hepatic duct; from thence it paſſes through the ductus communis choledochus, and is finally diſcharged into the duodenum. This is the moſt common courſe of the bile, but if from any ſpaſmodic affection or morbid diſtention of the duodenum, or from any obſtruction in the ductus communis choledochus its paſſage ſhould be prevented, a retrograde motion of this fluid is the conſequence. In this caſe it regurgitates through the cyſtic duct and finds its way into the gall-bladder, which is a very convenient receptacle, deſtined to prevent the hepatic duct from being ſurcharged with bile, in caſes either of obſtruction or of too great ſecretion.

Cyſtic bile differs, it appears, from hepatic, in this, that its conſiſtence is more grumous, it is of a darker hue, and has a more pungent bitter taſte. They do not differ in their conſtituent parts, but only in the proportion of thoſe parts. In fact

I do believe, that the only material difference which exiſts between them, depends on the more aqueous part of the cyſtic bile being abſorbed, from its confinement and ſtagnation in the gall-bladder. Two cauſes concur to promote the diſcharge of cyſtic bile. The one is, the preſſure which the gall-bladder receives from the neighbouring parts, and particularly from the ſtomach when diſtended with food; the other, that either from the acrimony which bile acquires by ſtagnation, or from the mere ſtimulus of diſtention, a contraction of the muſcular coat of the gall-bladder will be induced, and its contents will thus be readily propelled into the duodenum.

From the experiments of the moſt accurate chemiſts, the conſtituent parts of the bile appear to be, 1. A coagulable lymph, 2. A reſinous matter, 3. Animal gluten, 4. Soda, and 5. A colouring matter, which is believed to be iron. Neither of theſe component parts of the bile is preſent in a perfectly free and diſengaged ſtate; they are all ſo combined as to form an apparently homogeneous fluid. Bile, without the aſſiſtance of an intermedium, is not miſcible, as far as I know, with any volatile or fixed oil, with animal fat, nor yet with butter or any other oleaginous ſubſtance. Neither does it appear to render theſe ſubſtances miſcible with water, although perfectly ſo itſelf. I have ſeen ſome of the bile of an ox, which had been mixed in a phial with an equal quantity of olive oil, and which after having been kept for more than twelve months and frequently agitated, had not the leaſt diſpoſition to unite with it. The idea of its rendering oils miſcible with water, appears to have ariſen from its being long ſince uſed in the cleaning of ſtuffs, and hence it has been ſuppoſed to act chemically and in the ſame manner in which ſoap does. It does I believe act chemically, but ſtill not in exactly the ſame manner in which ſoap does. Soap is ſaid to have an attraction for both oil and water, and thus renders them miſcible; whereas, bile by having a greater attraction for the ſtuff to be cleaned, than oil has, only tends to diſplace it, and it is in this way that I believe it always acts, when uſed to remove ſpots of oil, or other greaſy matter, from ſubſtances to which they are adherent.

The uſe of the bile in the animal œconomy is evidently material, and I may add, it is ſingularly important in the proceſs of digeſtion. I do not however believe, that it has any effect in this proceſs while going on in the ſtomach, as its preſence in this organ is the conſequence of regurgitation, and is without doubt morbid. This I infer, firſt, from the ſickneſs of ſtomach, vomiting, vertigo and other ſymptoms of great morbid action, which attend its preſence in that organ. And ſecondly, from theſe ſymptoms being ſpeedily relieved, by ſuch medicines as effect its diſcharge.

Doctor Monro having caught ſeveral frogs, at different times killed three of them, and as ſpeedily as poſſible emptied the contents of their gall-bladders into the ſtomach of another. The conſequence of this was, that the animal died ſhortly after.* I do not attempt to prove any thing more from this, than that there is ſomewhat deleterious in the bile of theſe animals, particularly as we are aware that the bile of other animals have been taken not only with impunity, but even with advantage, I have poured the recent cyſtic bile of a cat down the throat of a puppy, without his ſuffering the leaſt inconvenience from it; and I have ſeen a black ſervant whoſe taſte had become ſo vitiated, after having accuſtomed himſelf to take the bile of an ox frequently as a ſtomachic, that he became fond of it, and ſo far from its proving injurious to him, he always thought himſelf much better after uſing it.

Bile neutralizes both the vegetable and mineral acids and is itſelf decompoſed by them. In the duodenum, by mixing with the chymous maſs diſcharged from the ſtomach, it firſt begins to ſeparate the chyle from it, and being itſelf decompoſed, its more aqueous part unites with the chyle, while its reſinous adheres to the fæces giving them their natural yellow colour; thus the former is prepared to undergo the round of the circulation, the latter to be diſcharged from the body.

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* Dr. Wiſtar's Lectures.

"A further uſe of the bile," ſays an admired author, "is, to evolve and exterminate from the "alimentary canal, the fixed air, which had been "hitherto confined among the chymous maſs."* Again, it prevents the too great accumulation of mucus in the inteſtines, and by its ſtimulus increaſes their periſtaltic motion. Hence it is that biliary obſtructions are generally accompanied with coſtiveneſs, and ſometimes with a diſcharge of mucus.

The property of being both powerfully antiſeptic and antizeumic, is likewiſe ſaid to be poſſeſſed in an eſpecial manner by the bile. Maclurg tells us, that "this fluid after having remained to his "knowledge, for three days in a dead body, and "although when the gall-bladder was taken out, "there was a very offenſive ſmell in all the abdo- "minal viſcera; yet this fluid, being poured into "a phial, and cloſely ſtopped, acquired a ſweet "ſmell, which continued ſome days before the "putrid fetor began."† This property of bile has been ſuppoſed to be intimately connected with its bitterneſs. Knowing, however, that bile is ſecreted from the dark coloured blood of the vena portarum, (which is entirely free from any changes effected by air, through the medium of the lungs, and in fact poſſeſſing all the characters of common venous blood,) does it not appear probable, that its antiſeptic quality depends on its attraction for, and conſequent abſorption

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[*] Blumenbach.

[†] Vid. Treat. on the Human Bile. p. 76.

abſorption of, oxygene. The opinion that the blood, by its circulation through the abdominal viſcera, receives a putreſcent tendency, appears to be erroneous, as it is deprived of its oxygene and conſequently becomes antiputreſcent; for bodies can only become putrid by the abſorption of this gas.

The difference which exiſts between the blood of a fœtus, which is appropriated to the ſecretion of bile, and that of an adult, appears to be worthy of ſome attention. The difference is briefly this, the blood, from which the bile of a fœtus, is ſecreted, partakes more of the quality of arterial blood than that of an adult; this likewiſe makes a proportionate variation in the properties of the bile, and conſequently that of a fœtus is of a more putreſcent nature, or in other words, has leſs tendency to reſiſt putrefaction.

The quantity of this fluid ſecreted ſeems evidently to be great, particularly when we conſider the vaſt apparatus of its ſecretory organ, and the quantity of blood conveyed to it for this purpoſe. Indeed Dr. Monro tells us, that four ounces of cyſtic bile have flowed through an ulcer of the ſide daily.*

5th. *Of the Pancreatic Juice.*

The juice denominated pancreatic, is a limpid fluid, which bears a greater reſemblance to ſaliva than to any other fluid in the human body. It is ſecreted from a long and flat gland of the conglomerate kind, which lies under the ſtomach, and between the liver and the ſpleen, and which is known to anatomiſts by the name of the pancreas. The ſituation of this gland in the abdomen is tranſverſe, being in the duplicature of the poſterior portion of the meſocolon. It is found not only in man, but in moſt other animals, in quadrupeds, in birds, and in many fiſhes.

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* Vid. Syſtem of Anatomy. Vol. II. p. 389.

The external appearance of the pancreas, is that of one uniform ſubſtance, with its ſurface ſomewhat uneven from a conſiderable number of ſmall convexities, and reſembling very much in its ſtructure the ſalivary glands. In the centre of the breadth of this gland, we find its great duct running in a longitudinal direction, and into which ſeveral ſmaller ones empty themſelves on each ſide, like ſo many minute branches inoſculating with one parent ſtalk. It has very properly gotten the name of the pancreatic duct, and opens generally in common with the ductus communis choledochus into the duodenum, for the purpoſe of emulging its contents. This however is not always found to be the caſe, as it ſometimes opens by a ſeparate duct into that inteſtine.

An exact analyſis of the pancreatic juice, as far as I know, has never yet been made; but, like moſt of the fluids of the human body, it is found to contain common ſalt and ſal-ammoniac. The difficulty of procuring a ſufficient quantity of this fluid, I believe, is the reaſon, why its analyſis has not hitherto been much attended to. We are directed to obtain it by inſerting a ſmall tube, to which a phial is appended, into the pancreatic duct of a living animal; but this cannot be ſo readily accompliſhed, as we may at firſt ſight imagine.

The uſe of this fluid is not perfectly underſtood. It is generally believed to have the effect of diluting the chymous maſs, after its having paſſed into the duodenum from the ſtomach, and to aſſimilate it to an animal nature. Likewiſe to dilute and attenuate the bile, which is ſometimes too viſcid and acrid.

From analogy the quantity of this fluid ſecreted appears to be very great; as the pancreas is no leſs than three times as large as the ſalivary glands, and has every circumſtance as favourable for the ſecretion of its fluid. Like other glands in the body, its ſecretion is increaſed by ſtimulants, which no doubt makes a very conſiderable variation in the quantity ſecreted at different times. The preſſure which this gland receives from the ſtomach when diſtended with food; the irritation of the chime in its paſſage into the duodenum; and even that of the bile itſelf, tends to promote the diſcharge of its juice. Like the bile, Haller ſays,* it is capable of being regurgitated into the ſtomach.

SECTION

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* Element. Phyſiol. Tom. VI. page 309.

SECTION III.

HOW FAR SOLUTION IS ADMISSIBLE IN DIGESTION.

TO aſſert that ſolution does not take place in digeſtion, would be to deny every thing like teſtimony in medicine. My attempt will only be to ſhew, why it is not probable that it can be the ſole efficient cauſe of that proceſs, and how it ſhould be conſidered as tending to promote it.

By ſolution we can comprehend nothing more, than ſo minute a diviſion of the particles of any matter, as to render that matter capable of being diſſolved in a fluid; and this too, without effecting any change of its component parts; for no poſſible diviſion be it ever ſo minute can have this effect. Thus, the component parts of water are the ſame, whether it be in the ſtate of ice or of vapour. If this be a correct definition of what we underſtand by the term ſolution, a ſolvent in the ſtomach can have no other effect on our food, than merely that of ſeparating it into very minute parts or particles; but this is very far from being all which takes place in digeſtion. Such a change muſt be effected on our aliment, for the due nouriſhment and ſupport of our ſyſtems, as to convert it into that mild and bland fluid which we denominate chyle. This fluid differs in both its appearance and properties from the matters taken into the ſtomach, from which it was prepared, and conſequently cannot be the effect of mere ſolution, by which operation matter may be divided, but I preſume can never be changed in its component parts. Again, chemiſtry has not yet taught us that any difference exiſts between the chyle of carnivorous and that of herbivorous animals, and from the moſt direct experiment we learn, that two animals of the ſame ſpecies being fed, the one on the matter of a muſcular fibre, and the other on farinaceous matter, both afforded chyle in no reſpect different from each other.* Whereas, could chyle be produced by mere ſolution, it ſhould ſurely differ in its properties, in proportion to the variety of matter from which it is prepared.

From theſe facts it appears, that the aid of ſome other operation is requiſite to explain the formation of chyle. Perhaps ſeveral may be found neceſſary. In addition to ſolution, I do believe, that fermentation has likewiſe a very conſiderable effect. By it we know, bodies not only become decompoſed and reduced to their elementary principles; but, by a recombination of thoſe principles another ſubſtance is formed, differing materially from that from which it was obtained. It is by a ſimilar decompoſition and recombination of the elementary principles of food, together with its ſubſequent mixture with certain fluids of the primæ viæ, that I believe it becomes ſo far animalized and changed it its properties as to form chyle. That by the combination of bodies another is formed, differing in its properties from either of thoſe of which it is compoſed, is too true to be denied. Thus, if we combine with a proper proportion of hydrogene and carbone, a certain proportion of oxygene, we obtain ſugar, a ſubſtance differing very eſſentially from either of the other three. The effect of ſolution not being that of a change of the component parts of food, it is clear that its only operation in digeſtion muſt be, that of expediting fermentation. This opinion, if we judge from analogy, I preſume will be found correct. In ſimilar circumſtances out of the body, the more minutely any matter is divided which is capable of fermentation, the more ſpeedily will that matter go through its ſeveral ſtages. To return to our ſimile of ſugar. If we diſſolve this ſubſtance in water, its particles may be ſo minutely divided as not to be perceptible in that fluid, yet by evaporation the ſame ſugar may be obtained, not at all changed in its properties. But if we ſuffer ſugar to ferment, the reſult will be very different. It will be reſolved into its elementary principles, carbonic acid will be diſengaged, and we will likewiſe obtain alcohol. Here then is a decompoſition and recombination of its elementary principles. Sugar being compoſed of oxygene, hydrogene, and carbone, and theſe elements being ſeparated by fermentation, are re-united to form theſe two ſubſtances, to wit, the oxygene unites with a part of the carbone and is diſengaged in the form of carbonic acid, while the remainder of the carbone is diſſolved by the hydrogene and forms the alcohol. Thus we ſee the material difference in the effects of ſolution and fermentation.

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* Fordyce on Food, p. 143.

SECTION IV.

WHY IT IS PRESUMABLE THAT FERMENTATION TAKES PLACE IN DIGESTION.

WE have already conſidered digeſtion *a priori*; that is, we have inveſtigated the cauſes which influence digeſtion. We have taken notice of the ſeveral properties of the matters which have the moſt material effect in this proceſs. And we have likewiſe ſeen, that all the circumſtances eſſential to fermentation, are poſſeſſed in an eſpecial manner by food in the ſtomach. We have ſeen, for inſtance, that it is plentifully ſupplied with moiſture, not only from our drinks, but even from our ſaliva and the fluids of the ſtomach itſelf; that it receives a ſufficient quantity of air from our ſaliva, by which fluid it is enveloped and continually conveyed into the ſtomach; and laſtly, that its ſituation is admirably adapted to be ſupplied with the neceſſary quantity of heat; on all of which circumſtances fermentation in a particular manner depends. We have alſo ſhewn, that ſaliva and the mucus of the primæ viæ, have a conſiderable tendency to promote this proceſs.

Having proceeded thus far on our ſubject, it next becomes neceſſary, that we conſider digeſtion *a poſteriore*; or, in other words, that we attend to the effects produced by the digeſtion of food in the ſtomach. But in the firſt place, we ſhall ſay a few words on fermentation.

Fermentation is that great agent in nature, by which bodies are rendered totally different in their chemical properties, and which, from the variety of its products, has been long ſince divided into three ſeveral ſtages; to wit, the vinous, the acetous, and the putrefactive. From the firſt of theſe proceſſes we obtain, *alcohol*; from the ſecond, *vinegar*; and from the third, *ammoniac*; by which means we are able always to aſcertain, the nature of whatever fermentation has taken place.

It will be recollected, that theſe ſeveral ſtages of fermentation are capable of taking place, entirely independent of each other. Whenever the ſaccharine principle of any matter predominates, the vinous fermentation will take place; when the mucilaginous is moſt abundant, it will undergo the acetous; and when a greater proportion of gluten is preſent, it will run immediately into the putrefactive ſtage of fermentation. From this it appears, that on the ſeveral proportions of ſaccharine matter, of mucilage and of gluten, which any ſubſtance contains, depends the priority of the fermentation which will commence. Thus it is, that the vinous fermentation is capable of preceding the acetous, and *vice verſa*. But they do not neceſſarily follow each other. Hence it is, that old and generous wines, in which the mucilaginous principle had been deſtroyed, are no longer capable of becoming acid, without the addition of a certain proportion of gummy matter.* Neither does milk afford a vinous ſpirit by its own ſpontaneous change, as in this caſe it loſes its ſaccharine principle.

Different gaſes are diſengaged, during the progreſs of theſe ſeveral ſtages of fermentation. The nature of theſe depend on the matter fermented. Thus, in the vinous and acetous ſtages of fermentation, carbonic acid is diſengaged; while in the putrefactive, azote, carbonated hydrogene gas, ſulphurated hydrogene gas, and phoſphorated hydrogene gas, are all occaſionally evolved.

We will now conſider how far theſe gaſes, as well as the other products of fermentation, can be conſidered as being evolved in the primæ viæ.

In ſupport of the firſt of theſe poſitions, we have that common fact, of our perceiving conſiderable eructations of air in affections of the ſtomach. Here it may be ſaid, that the diſengagement of air is the conſequence of diſeaſe. To this I anſwer, that the eructation I conſider as ſuch, but not the formation of air. In affections of the ſtomach from gout, the quantity of air diſengaged is ſometimes very great, yet we cannot ſuppoſe that it is formed by this affection. I regret that I cannot from my own experiments, ſay any thing relative to the nature of the air, which is evolved in the ſtomach during digeſtion. To place this matter, however, in as clear a light as I am able, I ſhall take the liberty of making a quotation from Plenck.* This gentleman informs us, that, " in a very healthy man, frozen " to death by cold on a winter's night, there was " found a mixture of four kinds of air in the primæ " viæ.

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* Chaptal's Chem. Vol. III. page 268.

" Fixed air was found in the greateſt quantity in " the ſtomach, and but little in the ſmall inteſtines.

" Vital air was contained chiefl iny the ſtomach, " and ſmall inteſtines, and,

" Azote, and carbonated inflammable air, in " the large inteſtines."

To theſe I may add, that ſulphurated hydrogene gas, and phoſphorated hydrogene gas, have been proven to be diſengaged *in crepitu.*

It appears from the works of Van Helmont, that he was the firſt perſon who ſuggeſted the idea of the preſence of an acid in the ſtomach. His opinion has been aſſented to by Haller, who relates, that the acetous fermentation is very prevalent in the ſtomach; that an acid is ſpontaneouſly evolved before putrefaction, and even ſometimes reſiſts that proceſs. He indeed mentions an acid being detected in the ſtomachs of ruminating and of omnivorous animals, in thoſe of birds and even of carnivorous animals.*

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* Treat. on the human fluids, page 141.

Moſt perſons have witneſſed the preſence of an acid in their ſtomachs. But here an objection riſes. It has been ſaid, that whenever an acid is preſent in the ſtomach it is morbid, and indicates he diſeaſed ſtate of that organ. When accompanied with eructations, I believe this to be the caſe; as the quantity then appears to be preternatural, and is attended with an inverted periſtaltic motion of the ſtomach, which is certainly the effect of morbid action. But we muſt not infer from hence, hat the preſence of an acid in the ſtomach, is he effect of diſeaſe, as I hope to ſhew that it is detected in that organ, in the moſt ſound and naural health.

This has been proven by Dr. Ruſh† many years go. He has ſhewn us by ſeveral well directed xperiments, that he always detected the preſence f an acid in the contents of the ſtomach, when hrown up by an emetic, three hours after food was taken; but as it has been objected to his

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* Elemen. Phyſiol. Tom. VI. p. 140 and 141.

† Inaug. Diſſer. de Coctio. Cib. in Vent.

experiments, that the acidity proceeded from the emetic tartar, which had been decompoſed in the ſtomach, I ſhall endeavour to ſuperſede this difficulty.

A gentleman in perfect health and capable of ruminating, dined on roaſted beef, Iriſh potatoes and leavened bread. His drink was nothing but water. In four hours after, he brought up a portion of his dinner. It had an acid taſte, and turned a blue vegetable ſubſtance of a red colour.

The ſame gentleman having dined on boiled mutton, cabbage and leavened bread, and having drank water alone, as before, in four hours after, ruminated again. The portion of food brought up had an acid taſte, and, as the laſt, turned a blue vegetable ſubſtance of a red colour.

Theſe experiments were frequently repeated after having dined on different ſubſtances, and with uniform ſucceſs. It was obſerved, that the acidity was not as perceptible to the taſte in an hour or two after having dined, as it was in ſeveral hours after. Theſe experiments may be readily repeated, as I do believe, that almoſt any perſon with a little trouble, may learn to ruminate.

Again, to determine whether an acid is evolved in the digeſtion of a cat, one was fed on boiled beef and Iriſh potatoes. In five hours after, it was ſtrangled. Its abdomen was opened and its ſtomach taken out, having previouſly placed ligatures on its two orifices. The food had become ſoft and pulpy, and there was very little of any kind of fluid in the ſtomach. Some of this pulpy maſs being placed in a glaſs veſſel and mixed with a little water, ſhewed evident marks of acidity. It very ſpeedily reſtored the yellow colour of paper ſtained with rhubarb, after its having been turned brown by an alkali.

I am not ignorant of the aſſertion of Dr. Fordyce, that in his experiments on dogs, cows, and ſheep, he could not find the leaſt trace of acidity in the duodenum;* neither could it have been expected othewiſe, ſince, as has been already related, the bile has the effect of neutralizing acids, and of conſequence as that fluid is almoſt always flowing into the duodenum, the quantity of acid muſt be unuſually great to be detected in that inteſtine.

It is a prevalent opinion, that the acid which is preſent in the ſtomach is the phoſphoric, and conſequently that it is not the effect of fermentation. But, as it is our duty not to admit either one poſition or another, unleſs it is ſupported by facts or experiments, I ſhall relate ſuch of theſe as have induced me to preſume, that it is not the phoſphoric acid which is uſually found in the ſtomach. And,

1ſt. The acid found in the ſtomach, does not precipitate ſugar of lead from its ſolution in water.

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* Vid. Treat. on Food. p. 150 and 151.

2d. "Being ſaturated with kali, that is, what "was formerly called fixed vegetable alkali, it "produces kali acetatum, formerly called regenerated tartar, or ſal-diureticus."*

Deſirous of knowing whether an acid is evolved in the digeſtion of animal ſubſtances, as well as in the former experiments, the gentleman, capable of ruminating, dined on roaſted veal alone and drank water as uſual. In four hours after, a portion of the contents of his ſtomach was brought up. It turned a blue vegetable ſubſtance of a red colour, and had an acid ſmell and taſte. I confeſs my being at a loſs in this caſe to determine, whether the acid was the effect of the digeſtion of the veal, or whether it proceeded from the remains of ſome former food. It was my intention to have aſcertained this point, by repeating the experiment on the ſame perſon, after his having ſubſiſted on animal food for eight or ten days; but, as I have not now as many days previous to the delivering in of my piece, I ſhall be obliged of conſequence to decline the idea. The following experiment however, will at leaſt adduce probability, in favour of the acid having been evolved by digeſtion.

Having placed too drachms of roaſted veal in a glaſs veſſel, and covered it completely with ſaliva, it ſpontaneouſly became acid, long previous to there being any ſigns of putrefaction taking place. This fact is corroborated by the experiments of many authors of reputation. Haller takes notice of it.* Dr. Ruſh found that beef acquired an acid taſte and ſmell, when expoſed for two days to the heat of ſummer;† and Maclurg relates, that a mixture of mutton and water, paſſes through the acetous ſtage of fermentation before it putrefies.‡

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* Fordyce on Food. p. 148.

All animal matters when mixed with fermentable vegetable ſubſtances, have a tendency to promote fermentation, as appears from a number of experiments inſtituted by Pringle, and who likewiſe adds, that "after ſuch mixtures become ſour they never "return to a putrid ſtate, but, on the contrary, "grow more and more acid."§ And yet, ſo far from our finding this ferment to be injurious to digeſtion, on the contrary, animal food ſeems to be the beſt adapted for the aliment of dyſpeptic patients.

The publication of Dr. Wilſon's ingenious inaugural diſſertation on digeſtion, has induced many perſons to ſuppoſe, that every idea of fermentation taking place in digeſtion is unfounded. The Doctor ſubmitted to diſtillation, the contents of his ſtomach, brought up by an emetic four hours after food was taken, with a view of aſcertaining, whether he could detect the preſence of ſpirit of wine. This

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* Vid. Element. Phiſiol. Tom. VI. p. 316. † Vid. Inaug. Diſ. p. 21.

‡ Treat. on the bile. p. 75.

§ Diſeaſes of the Army, Appendix, paper 4, Experiment 28.

he could not, and from hence concludes, that fermentation does not take place in digeſtion. It will be ſeen that I have repeated the Doctor's experiment, and I am happy to add, with the ſame ſucceſs; but I ſhall not draw the ſame concluſions.

Being in perfect health, I dined on corned beef, potatoes and leavened bread. My drink was water alone. In four hours after, I took twenty grains of ipecacuanha and brought up the contents of my ſtomach. It ſhewed evident marks of acidity. On ſubmitting it to diſtillation, a tranſparent and limpid fluid came over into the receiver, which had a taſte ſomewhat ſweet, and an agreeable flavour; it had not the leaſt appearance of ſpirit of wine, neither could I detect in it the preſence of an acid. On examining however the *reſiduum*, which had not been evaporated to dryneſs, I was not a little ſurpriſed to find it ſtill acid.

A dog was kept faſting for twenty-four hours. He was then fed for two days ſucceſſively on animal food. Four hours and a half after taking his laſt meal, he was killed. The food in his ſtomach ſhewed evident marks of acidity. On ſubmitting it to diſtillation, I obtained, as in the laſt experiment, a tranſparent and limpid fluid, ſomewhat ſweet, with an agreeable flavour and in no reſpect different from that which I had obtained from the contents of my own ſtomach.

From the above experiments we learn, firſt, that an acid was produced in digeſtion as in the former experiments, and that it was even found in the reſiduum after diſtillation; and ſecondly, that ſpirit of wine could not be obtained from the food by diſtillation; but by no means that fermentation did not take place. I do not ſuppoſe that the vinous fermentation in a healthy ſtomach, is ever ſo complete that ſpirit of wine is formed, this would be equally as incorrect as to ſay, that putrefaction takes place in that organ. Neither can we ſuppoſe, that ſpirit of wine can ever be obtained from any matter, in which the acetous ſtage of fermentation has been completed and is then preſent.

We have already ſeen that the acetous fermentation is capable of preceding the vinous; it is probable this may be the caſe in digeſtion; nor would it be by any means ſingular. In the preparation of *koumiſs* from the milk of mares, a drink much in uſe among the Tartars, the acetous fermentation always precedes the vinous. But admitting that the vinous fermentation does have the priority, every circumſtance, to which our food is expoſed in the ſtomach, muſt tend to hurry it on ſo ſpeedily to the acetous ſtage, as to prevent the former from being at all perceptible. The heat to which it is expoſed in the ſtomach, is greater than that which is requiſite for the vinous fermentation; and this exceſs of heat favours the acetous ſtage.

When ſpeaking of the acid formed during digeſtion in the ſtomach, the word evolve has been uſed by many perſons; whenever I have followed them in this reſpect, it will be recollected that I have always meant, that it was evolved by fermentation. I do not ſuppoſe, that the acid in the ſtomach is ſimply diſengaged from our food, as it is from the common *ſumach* when mixed with water, or any other ſubſtance in which it is very abundant. The quantity of acid contained in the food, on which we have experimented, is not ſo great that this could have been the caſe; neither can it be obtained from it out of the body by any other means, than by the aſſiſtance of fermentation.

It now remains with the reader to determine, whether or not my poſition is correct, that this acid is the effect of fermentation.

Fermentation out of the body, differs from that which takes place in a living ſtomach, in this, that the former is ſpontaneous; whereas, the latter is induced by all the numerous circumſtances in the ſtomach, which tend to promote that proceſs, and of conſequence muſt be more ſpeedy,

Does not the fact of digeſtion being more ſpeedy while we are at reſt, than during exerciſe, favour the theory of fermentation?

How ſhall we account for that warmth about the region of the ſtomach, ſo perceptible in ſome perſons for ſeveral hours after death, unleſs we admit of fermentation?

Whether we ſhall ever be ſo ſucceſsful as to imitate nature in digeſtion, as in many of her other operations, I am unable to decide; but, I flatter myſelf, time, that correct diſcerner of truth and error, will direct the attention of ſome more fortunate experimenter to this ſubject, and diſſipate every doubt which may ſtill involve it.

Having thus delivered my obſervations and experiments on digeſtion, in as conciſe a manner as my time would admit of, I ſhall now cloſe this eſſay; but to do this without an acknowledgment to the ſeveral Medical Profeſſors of this Univerſity, would be a breach of that duty which my feelings claim.

To you, gentlemen, at leaſt this ſmall tribute of my eſteem is due. Permit me then to preſent you with the ſincere acknowledgments of a pupil, for the many opportunities of improvement which your lectures have afforded him. With the aſſurance of my wiſhes, that you may continue to enjoy all the pleaſure of ſucceſs in the ſcience of medicine, I now bid you, adieu.

FINIS.

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